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Interactive comment

Interactive comment on "Climatology and Interannual Variability of Dynamic Variables in Multiple Reanalyses Evaluated by the SPARC Reanalysis Intercomparison Project (S-RIP)" *by* Craig S. Long et al.

Anonymous Referee #3

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This paper provides a comparison of climatological aspects of the stratospheric temperature and wind fields produced by multiple reanalyses, covering the current and previous generations of product from the major providers. It is in general quite clearly written, and the discussion of results is even-handed in its treatment of the various reanalyses. It and companion papers from the S-RIP are likely to provide useful points of reference for users of reanalyses and some new information for producers of reanalyses, even though the latter may be quite well versed in the characteristics of their own products and more focussed on the performance of new systems that are either in production or being prepared for production. The paper merits publication, although

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it should at least be amended to address several issues noted below that can be easily fixed. The authors should also consider whether it is feasible to make any more substantial change in response to the comments below.

The paper lacks emphasis on the lower stratosphere, and related to this is a lack of demonstration of the benefits of assimilating GPSRO data. Many of the figures are dominated by the large differences in the upper stratosphere, but the smaller differences found lower down may be of importance for those undertaking studies of the UTLS region. Here differences of a few tenths of a Kelvin may be significant, and it has been found that assimilation of GPSRO data brings the reanalyses that employ it into much closer agreement. Of the three contributors to the authors' REM, ERA-Interim and JRA-55 do assimilate GPSRO data and MERRA does not. This has proved instructive, for example, in identifying reanalysis bias in tropical tropopause temperatures (as discussed in GCOS Publication 195, page 235). Further comments are given below in the discussion of section 6.2; note in particular the final comment on this section. Perhaps contour intervals that provide more detail where differences are small could be used, or the authors could consider using times series plots for a few selected levels as well as (or instead of) the pressure/time cross-sections.

What happens during the transition from the TOVS to the ATOVS systems needs to be properly explained. The existing text is wrong. It is incorrect to state that only some reanalyses "merged the observations . . . over a period of time [Page 5]" and that others "switched immediately from the TOVS to the ATOVS observations [Page 19]". ERA-Interim is one of the reanalyses with a sharp discontinuity during the changeover from TOVS to ATOVS, but it is due (as documented in several of the peer-reviewed papers on this reanalysis) to a change in the anchoring of the bias correction; ATOVS observations were assimilated from the time they first became available and TOVS observations were assimilated for as long as they were available in ERA-Interim. The discontinuity in ERA-Interim is indeed related to the shift from TOVS to ATOVS, but arises simply from changing the anchoring high-sounding channel for which bias-correction was not applied from SSU-3 to AMSU-A14. More generally, the assimilating models and the data from the high-sounding SSU and AMSU channels all have biases in the upper stratosphere, and the differences in structure functions between SSU and AMSU instruments complicate the picture. It is a continuing challenge to deal with these biases in the absence of independent data that reach above the levels reached by radiosondes. GPSRO data help a little bit, but do not reach much higher, and are not available in substantial numbers prior to late 2006.

The paper does not discuss synoptic characteristics of the temperature and wind reanalyses. I assume that these are being taken care of in other S-RIP papers, and that this will be obvious to the reader of this paper. Some cross-referencing might be helpful, however.

Page 1, lines 27 and 28. It is noted that the zonal winds are in greater agreement than temperatures, and that this agreement extends to lower pressures. This could be a consequence firstly of the temperature differences being predominantly in the global average, or at least of large horizontal scale, which is what one would expect if they are predominantly due to differences in radiance bias correction. Thermal wind balance would then suggest only small differences in wind fields. Moreover, if the temperature differences are of broad vertical scale, the associated thermal wind differences would be expected to be larger higher up, as the thermal wind is the vertical integral of the temperature gradient. Is this too simplistic an explanation of the authors' finding?

Page 2, line 23. "Fiorina" should be "Fiorino".

Page 4, lines 13-16. Is it appropriate to consider CFSR to be a single reanalysis? The fundamental idea of a reanalysis is that it uses a fixed data assimilation system to analyse past observations. The upgrading of the resolution of the assimilating model and of the data assimilation in 2011 seems contrary to this fundamental idea. What is the justification for treating CFSR as a single reanalysis? Would it be better to show CFSR results only until 2010? The CFSR results in Fig.1 and (for SSU) Fig. 10 suggest a change in behaviour of CFSR in the upper stratosphere around the end of 2010.

Page 6, line 6. “Sceptically” is not the word I would use. Reanalysis data have a part to play along with other sources of information for the study of climate trends, but that use has to be careful, selective, and guided by studies such as the one under review. For some purposes, discontinuities can be corrected for, as has been done for ERA-Interim in published work.

Page 6, line 19. I would write that the 1000hPa level is “under” rather than “over” the Antarctic and Tibetan plateaux.

Page 9, lines 21 and 22. The MERRA-2 model is stated to be the only of the assimilating models capable of generating a QBO on its own, but MERRA-2 has the poorest agreement with the Singapore radiosonde data at 10hPa for the 1980-1998 period. Why? Also, the remark that MERRA-2 winds are greatly improved versus those of MERRA after 2000 does not hold for the Singapore winds at 10hPa, as these are fitted better by MERRA than MERRA-2. Furthermore, it is stated earlier in the paragraph that zonal winds in the tropics are not well constrained by the assimilated radiances, yet the MERRA-2 winds are apparently much better in the ATOVS period than the TOVS period. This needs further discussion.

Page 10. Section 4.1 is introductory, but refers in the beginning to a figure relating to SH polar latitudes. This text would seem more appropriately located at the start of section 4.1.1. Results are shown only for polar and tropical latitudes. What about middle latitudes? Extra figures may not be necessary, but the situation in middle latitudes should at least be summarised in the text.

Page 14. Is it sufficient to compare the QBO winds only with the radiosonde data from Singapore? Are the authors confident that this gives a representative picture of the equatorial region as a whole? Did Kawatani et al. (2016) find much variation from one radiosonde station to another?

Page 16. The comparison with HIRDLS data is good to see, but again a summary should be given of what is found for middle latitudes.

Page 17. Several comments relate to section 6.2:

(i) The start of the section could be clearer. The opening sentence should include reference to the AMSU-A and ATMS channels that provide temperature observations in the middle and upper stratosphere, and give links to where their weighting functions can be found. SSU observations cease in 2006, so some of what is shown in Figure 15 must not be SSU observations, but rather the observational equivalents from AMSU-A and ATMS data. This should be clarified.

(ii) Why are SSU3 data and their equivalents not included in Figure 15? They are assimilated by the reanalyses.

(iii) The ERA-Interim-focussed paper by Simmons et al. (2014) is referred to earlier, but the reference could be repeated here. Simmons et al. included results on how well ERA-Interim fitted HIRS and AIRS data, two types of stratospheric temperature data that are not mentioned in section 6.6.

(iv) The reference to Seidel et al. (2016) should be checked, and amended if necessary. I believe it did not consider SSU trends for 1979-2015, as it did not use the AMSU-A and ATMS extensions of the SSU data record.

(v) It is stated (lines 18 and 19 on page 18) that the reanalyses begin to disagree more with each other after GPSRO data become available in 2006. It must be made clear that this statement refers to anomalies not the actual temperatures. Assimilating GPSRO data brings the reanalyses closer to each other in the near-tropopause region and the lower stratosphere. The reference period used for the calculation of the climatologies on which the anomalies are based includes only a few years during which GPSRO data are assimilated, and the differences in anomalies shown after 2006 are mainly an indication of the differences in climatologies for those reanalyses that assimilate

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(vi) The caption to Fig. 15 states that the anomalies are with respect to 1980-2010 climatologies. If this is a typo, it should be corrected. Otherwise the figure should be changed to use the standard 30-year period 1981-2010.

(vii) The SSU curves for MERRA-2 in Figure 15 show a substantial spike around the beginning of 1996. This needs some discussion.

(viii) The (probably erroneous) trend in ERA-Interim after 2006 noted in the last line of page 18 and the first line of page 19 was noted by Simmons et al. (2014), who identified the likely cause – an increasing use of radiosonde data that had not been bias-corrected.

(ix) One of the reasons one does reanalysis is to produce height-resolved estimates of the meteorological fields, drawing on the better vertical resolution of radiosonde and GPSRO data than is provided by satellite radiances. A reanalysis whose bias does not change sign near the tropopause may well fit the layer average TLS dataset more poorly than a reanalysis whose bias changes sign close to the tropopause, but may be no worse or even better than the other reanalysis when it comes to comparisons at discrete pressure levels. This appears to be what is happening here: as noted in the paper under review, it is indeed most noticeable that ERA-Interim does not fit the TLS dataset well, but the evidence discussed at length in Simmons et al. (2014), supplemented by more recent results such as that on GPSRO discussed in the second paragraph of this review, does not point to a specific problem for ERA-Interim in its representation of lower stratospheric temperatures, but does indicate that the temperature differences between ERA-Interim and either JRA-55 or MERRA change sign between 70 and 100hPa. The discussion of the TLS data fits needs to acknowledge the above in order not to give a misleading impression of the capabilities of different reanalyses for representing temperature trends and variations at lower stratospheric levels such as 70 or 50hPa. The authors should also consider calculating and including in the paper time series of the fits at various levels of the reanalyses to a bias-adjusted radiosonde dataset, such as one of those produced by Haimberger.

Page 19, lines 9 and 10. It would probably be better to refer to "changes in the biases of the data from the TOVS/SSU instruments" rather than "changes in the TOVS/SSU instruments." The main differences in bias from one instrument to another stem from differences in cell pressure, and from the gaseous composition of the cell, linked to the behaviour of the cell seals, rather than from changes in the basic design of the instrument.

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